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AGE AT IMMIGRATION AND SCHOOL PERFORMANCE: A SIBLINGS ANALYSIS USING SWEDISH REGISTER DATA

by

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Age at Immigration and School Performance:

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Abstract

There is a gap in school performance between native and immigrant pupils in Sweden. This article analyzes the role of age at immigration, which is believed to be an important determinant of this gap, since it is inversely related to the time spent acquiring Sweden-specific skills before graduation. The analysis exploits within-family variation in a large set of register data on immigrant siblings (and native children) graduating from compulsory school between 1988 and 2003. The estimated negative impact from short duration of residence prior to graduation is significantly less than the one observed using a standard cross-sectional approach which fails to net out family-fixed effects. The critical age at arrival is about 10. Above this age, there is a strong negative impact on performance, where the sibling-difference estimates are 27-54 percent less negative than the cross-sectional ones. The results show both similarities and striking differences between boys and girls and between children of different origin. Moreover, children with short duration of residence perform significantly better in mathematics than in a range of subjects taken together. This demonstrates the importance of Sweden-specific skills.

Keywords: age at immigration, age at arrival, school performance, siblings approach

JEL codes: I29, J15

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1. Introduction

The gap in school performance between native and immigrant children in Sweden is well documented.[1] In 2003, for example, 91 percent of the native pupils received final grades from compulsory school that qualified them to continue at the upper-secondary level, while the corresponding number for children arriving in the country after the start of first grade is 64 percent.[2] Academic achievement at young ages is important for the acceptance to the upper-secondary level, but also for children's opportunities to successfully compete for admittance to the most popular study programmes within this level. Consequently, this gap is a real concern since it predicts a gap also in educational attainment, labor market performance and other long-run outcomes.[3]

The school performance gap is due to many factors. An important one is the lower general level of proficiency in the Swedish language and of other Sweden-specific skills (i.e., Social Science, History, etc.) among immigrant children. The lack of such skills is naturally more pronounced among children that are born abroad than among second generation immigrants and likewise in a comparison between children arriving after and before the start of first grade. Improving the educational achievement of children born abroad might require additional resources and new policies aimed at increasing the rate of acquisition of Sweden-specific skills.[4]

Given that the time to catch up on necessary skills before graduation falls with age at immigration and given that special directed policies are costly, knowledge about the impact of age at immigration should be crucial in order to target resources in an effective way. Two aspects are of particular importance and may serve as guidelines. First, it is important to examine the critical ages at arrival where effects on performance become significant. Since the variance in performance is large within the population of immigrant pupils, it is also relevant to examine this

[1] E.g., Lund et al. (2002)
[2] The Swedish National Agency for Education (2005).
[3] Chiswick and DebBurman (2004) find a strong negative association between immigration during the teenage years and educational attainment in the US.
[4] The current strategy to facilitate the adaptation process consists essentially of three parts. First, newly arrived children are offered a special introductory class where some fundamental Swedish is taught. Second, immigrant children have the option to study Swedish as a 2nd language instead of attending the ordinary Swedish class. Third, native language instruction is offered. See The Swedish National Agency for Education (2002) for details.

critical age separately for children of different origin. Likewise, the gender gap in school performance motivates studying boys and girls separately.[5] Second, the larger the negative effects on performance, starting at the critical ages and above, the larger the potential for significant improvements from directed resources aimed at speeding up the adaptation process. That is, the steeper the average age-at-immigration-performance-profile within a particular immigrant group, the larger the possibility that such resources could make a significant difference.

Using high-quality register data on compulsory school graduates between 1988 and 2003, this study examines the relation between age at immigration and a measure of grade point average (GPA) from the ninth grade. The main purpose is to estimate the effects from different numbers of years resided in Sweden prior to graduation on GPA and to examine the existence of critical ages at arrival where effects become significant. As an alternative outcome variable, final grades in mathematics are used as comparison since this subject is likely to require less Sweden-specific skills than most other subjects.

With this objective in mind, one has to consider the omitted-variable problem in estimation. Standard cross-sectional estimates might be seriously biased due to unobserved cohort-effects similar to the ones stressed by Borjas (1985) in the literature on economic assimilation among working-age immigrants.[6] The composition of the immigrant flow changes over time. In some years it might be dominated by families whose children do well in school even with a short time in Sweden prior to graduation while in other years it could be the other way around. Thus, estimated effects from different durations of residence might also reflect differences across immigrant cohorts regarding the children's possibilities to perform in school irrespective of the time they spend in the country prior to graduation.

There is also a selection problem involved. In making the migration decision, parents might take their children's ages into account, which then could give rise to selection bias if the way parents

[5] The gender gap in school performance in Sweden is documented and analyzed by The Swedish National Agency for Education (2004) and by Holmlund and Sund (2005).

[6] There is a large literature in the economics discipline studying outcomes for the adult immigrant population (e.g., Chiswick (1978), Borjas (1985), LaLonde and Topel (1992), Borjas (1999), Edin et al. (2003), Card (2005)) but surprisingly few studies pay attention to the assimilation or adaptation process of immigrant children (e.g., Gonzales (2001), Cortes (forthcoming), van Ours and Veenman (forthcoming)).

make such considerations correlates with omitted variables capturing family characteristics that are also related to the outcome. For instance, parents who have the choice to consider their children's ages when making the migration decision (e.g., labor-market immigrants) are more likely to migrate with young children compared to parents who do not have this choice (e.g., refugees).

In this study, an attempt is made to get around these problems by using a siblings approach and controlling for fixed family effects. The approach uses the within-family variation in age at immigration and treats an individual's siblings' outcomes as counterfactual outcomes for different years of residence prior to graduation. By doing this, the coefficients are identified by comparing individuals who belong to the same immigrant cohort, who share the same reason for immigration and who share the same socio-economic, cultural and linguistic background. Thus, any such observable or unobservable variables capturing permanent family characteristics shared by all siblings (which most commonly also include school affiliation) are held constant in the estimated regression. Standard cross-sectional analyses are also performed as comparisons.

This study not only provides new Swedish evidence of high policy relevance. It also makes a significant contribution to the international immigration literature.[7] The richness of the data allows for adopting the siblings approach on a problem it is well suited for. As a first application (to my knowledge) on this issue, the present study is of methodological interest since it uses an alternative approach to deal with the well known problems of omitted variables and selection so common in empirical research on immigration.

The paper proceeds as follows. Section 2 describes the data and discusses the empirical strategy. Section 3 shows some descriptive statistics along with a few background analyses. The main results are presented in section 4. Sensitivity analyses are carried out in section 5. Section 6 concludes.

[7] The evidence from this study adds to the previous Swedish evidence in Arai et al. (2000) and Similä (1994).

2. Data and empirical strategy

2.1. Data

The data stem from national registers administrated by Statistics Sweden. The basis for the specific data set used is a 20 percent random sample of each cohort born in Sweden and born abroad between the years 1972-1987. Adopted individuals are excluded. A second set of register data, (*flergenerationsregistret*) containing information on the full biological siblings to the randomly sampled individuals, has been matched to the first. A third register (*Årskurs 9 registret)* has been used to obtain information on grades from the last year of compulsory school.[8] These are available for the years 1988-2003. Data on immigrant siblings from this combined data set are used in the main analysis, while data on native children only are used to determine the grade distribution in separate subjects.

Henceforth, the following notations refer to data on immigrants alone: "random sample" denotes children from the 20 percent random sample; "sibling sample" denotes siblings to children in the random sample; "family sample" denotes the conjunction of the random and the sibling sample net of "lone-children" in the random sample (who do not identify any coefficients when estimating sibling-differences). The random sample contains 17,879 individuals (including 9,616 lone-children and children whose siblings not are present in the school data 1988-2003). The sibling and the family samples contain 13,782 and 22,045 individuals, respectively. By construction, children in the random sample are all born abroad, while their siblings may be born either abroad or in Sweden.

In using grades as a measure of school performance, it is important to only consider subjects compulsory to all pupils and that are given at the same levels of study at the same point in time. Unfortunately, the subjects of Swedish, Math and English fail to fulfill these criteria. Swedish is problematic since immigrants have the option to study their native language as their first and Swedish as their second language, where the latter then is given at a lower level than the ordinary

[8] A small number of pupils (about 0.1 percent of the sample) had grades from two years. These individuals were deleted along with the very small number of students that have attended private schools with a certain pedagogical profile which give grades that are different from the ordinary grade system.

teaching in the subject. Math and English are similarly problematic as they are given at two levels for all pupils until 1998. The GPA measure used as the outcome variable is, instead, constructed as an average of the grades in eight mandatory subjects which all are given at the same level during the whole period 1988-2003. These can be divided into two categories: Science (Physics, Chemistry, Technology and Mechanics, and Biology) and Social Science (History, Religion, Social Studies and Geography).[9]

The grades in each subject are given by the student's teacher on a 1-5 scale before 1998 and on a 1-4 scale since 1998. In order to obtain comparable estimates for the whole period, the grade data are transformed into percentile ranks which are determined per graduation year and by the 20 percent random sample of natives. Missing grades in separate subjects (for reasons such as too few hours of attendance to earn a grade) among graduates in the grade 9 registers are coded zero prior to the transformation.[10] Immigrant children are then assigned the rank that their grade (or missing grade) in each subject (in their year of graduation) would qualify them for in the natives rank distribution. From these percentile ranks, indicating an immigrant pupil's relative position in the natives' grade distribution in each subject, an average percentile rank is calculated, giving the same weight to Science and Social Science.

Mathematics has been given at the same level of study for all pupils from 1998 and onwards making it possible to use grades in this subject as an alternative outcome for the years 1998-2003. Grades in math are transformed into percentile ranks in the same way as described above for the GPA measure. The aim is to study if the estimates obtained using this alternative outcome differ significantly from the ones obtained using the GPA measure on the same shortened sample. This is particularly interesting since mathematics most likely requires less Sweden-specific skills than most other subjects. If one believes Sweden-specific skills to be a major factor explaining the gap

[9] Björklund, Lindahl and Sund (2003) construct their GPA measure in a similar way. More information on this as well as on the data from the national registers can be found in the same reference.

[10] A missing grade, thus, indicates the lowest level of performance, lower than the grade which indicates fail (which at least requires a minimum amount of attendance to be received). Treating missing grades in this way is reasonable for the purposes of this study because missing grades in mandatory subjects gives the information of failure (reasons for missing grades are indicated in the grade 9 register), and while the grade which indicates fail at least requires some effort a missing grade requires none.

between children with shorter and longer residences in Sweden, then one should expect the estimated gap to be larger using the GPA measure.

Turning to the explanatory variable of interest, the data set includes information on date of birth, year of graduation and date of immigration. Thus, we have the choice of either using age at immigration as the explanatory variable or a duration variable measuring years since immigration at time of graduation. Since the outcome is observed at the time of graduation when the individuals are about the same age, years since immigration and age at immigration are like two sides of the same coin. Both choices should capture the same variation between individuals. It, therefore, seems natural to base the decision on data accuracy.

Date of immigration is the crucial information involved in constructing both variables and is discussed in more detail below. Date of birth, necessary to construct the age at immigration variable, might be measured with error. Year of graduation, on the other hand, is highly reliable information. Based on this, the variable "years since immigration", defined as the difference between year of graduation and year of immigration is chosen as the regressor of interest. Interpretations of the regression results in terms of age at immigration are made as the difference between the normal graduation age and the number of years resided in Sweden.

Date of arrival in Sweden is the key variable for the purposes of this study. This information is based on the date of residential registration (*folkbokföringen*), which is administrated by the tax authorities. Newly arrived immigrants are registered with the aid of the immigration authorities. This variable, however, is not completely free from measurement errors. Individuals might, for instance, have different durations of residence in the country before they are registered.

It is well known that measurement errors in an explanatory variable generally lead to attenuation bias in cross-sectional estimation and that the problem increases if one uses differenced data. There are a number of studies discussing and analyzing the enhanced problem of measurement error bias in twins and siblings studies.[11] The problem of measurement error bias is much less of

[11] E.g., Griliches (1979), Ashenfelter and Krueger (1994), Bound and Solon (1999), Card (1999), Isacsson (1999), Neumark (1999).

a concern in this study. The reason for this is that measurement errors, to a large extent, should be the same for all siblings in a family. Since the individuals studied are children, they migrate along with their parents; they arrive in Sweden together and are registered in the residential registers at the same time (given that they all start off living together). This is done family-wise with the aid of the immigration authorities. Family specific measurement errors imply that these are fixed effects and, as such, netted out in the estimated regression. Specifically, the signal to noise ratio will be equal to one since the noise component is netted out. Hence, estimated coefficients will not suffer from measurement error bias.[12]

2.2. Empirical strategy

The idea of the siblings approach is captured in the following equation:

$$GPA_{ij} = \alpha + \mathbf{y_{ij}}'\boldsymbol{\beta} + f_j + \varepsilon_{ij}, \qquad (1)$$

where GPA_{ij} is the grade point average of pupil i in family j as defined in the previous section, α is the intercept, $\mathbf{y_{ij}}$ is a set of dummy-variables indicating immigrant pupils' number of years resided in Sweden at time of graduation, f_j captures unobserved family characteristics common to all siblings within the same family and ε_{ij} is the error term. The dummies in $\mathbf{y_{ij}}$ range from one, or less than one year, up to eleven years and having more than eleven years constitute the reference alternative.[13] By using regression-techniques estimating family fixed-effects, the potential impact that the factors in f_j have on the outcome is held constant in the model. Identification of the coefficients in $\boldsymbol{\beta}$ thus relies upon the sibling variation in the number of years resided in Sweden. The same coefficients should then, using this approach, not be subject to any bias due to

[12] Out of the 8,263 families in the family sample, 7,253 (or 87.78 percent) are families in which all siblings were registered during the same calendar year (and are, therefore, likely to have arrived together). 12.22 percent of the families include siblings differing in calendar year at arrival. These are necessarily not all families arriving in Sweden at different points in time. About 50 percent of these families include children from the sibling sample that are, in fact, born in Sweden and then assigned their year of birth as their year of immigration. An examination of how sensitive the estimates are for the inclusion of families that demonstrate differences in year at arrival is presented in section 5.

[13] Having more than eleven years means that the immigrant pupil is either born in Sweden or has arrived in the country before the start of pre-school.

influence from unobservables captured in f_j that are also associated with the outcome. The siblings approach is performed using the family sample.

The use of the siblings approach is motivated by the fact that immigrants to Sweden are an extremely heterogeneous group. Heterogeneity in the immigrant flow is likely to bias cross-sectional estimates. The composition of the immigrant flow has followed the trend of increasing shares of refugee immigrants and decreasing shares of labor immigrants over the past three decades (Lund et al., 2002). The year to year variation is also considerable. Immigrant children born between 1972 and 1987 constitute a very heterogeneous selection of the Swedish population.

The possibilities and incentives for young immigrants to perform well in school are likely to vary between different immigrant groups due to reason for migration, cultural and linguistic factors, and to average socio-economic background. For instance, the incentive to learn the Swedish language may be lower among immigrants that see themselves as temporary guests in the country and that plan to re-migrate. Thus, if one intends to identify the coefficients on years since immigration in the regression on GPA by using the variation between individuals graduating (and born) in the same year, but arriving in the host country at different years, the coefficients might reflect differences in the children's possibilities and incentives to perform irrespective of their age at immigration.

The use of a siblings approach can also be motivated by the presence of self-selection into the sample being analyzed. Parents who have the will and the possibility to consider the ages of their children when making the migration decision are likely to be overrepresented among those who migrate with young children. These parents are also more likely to hold characteristics that are good for their children's performance.[14] Parents who are considering whether or not to migrate and who also care a lot about their children's education should be more likely, if given the choice, to migrate at a point in time that is as favorable as possible for their children. This should be when they are young and not rooted in the source-country. The fact that parents consider the

[14] In contrast to this assumption, van Ours and Veenman (forthcoming) argue that age at arrival is exogenous for children since they merely follow their parents.

ages of their children when deciding to migrate will induce selection-bias. For example, labor-market immigrants often have this choice while refugees generally do not. It is also possible that parents who themselves are young when they migrate, and then also are more likely to have young children, are over-represented among the high-ability, well-educated and career-oriented immigrants.

Neighborhood effects constitute another potential source of bias. Some schools might be better than others at speeding up the adaptation process, making it less difficult for their immigrant pupils that only have a short time to catch up prior to graduation. This might be considered a problem in estimation since immigrant children are not randomly assigned to cities, neighborhoods and schools (and parents' choices are involved here as well).

In what directions should we expect these problems to bias cross-sectional estimates? The immigration flow (especially the flow of refugees) is characterized by shocks, e.g., the large numbers of people seeking asylum as a consequence of the Balkan wars in the early 1990's. Thus, by studying which groups dominate the identification of the coefficients on the variable years since migration for a single year cross-section, it would perhaps be possible to guess the direction of bias. For example, observing the outcome in 1994 would imply that refugees from the former Yugoslavia would be overrepresented among children with short duration of residence up to graduation. It could be that this group of immigrants had low incentives to invest in Sweden-specific skills, since many of these families planned to return as soon as the war was over. If this was the case, it could have pushed down the average results among children with high ages at migration graduating in the year of 1994.

It is of course less likely that there exist any general direction of this kind of bias if one studies a range of cross-sections pooled together. The selection problem, however, leads to an overestimation of the expected negative effects due to short durations of residence. That is because children who have a longer residence in the country prior to graduation also are more likely to have other characteristics that are good for school performance and vice versa (following the reasoning above).

The approach adopted in this paper exploits the variation between siblings in order to identify the coefficients of interest. Applying this strategy will net out any omitted variables capturing time-invariant, family-specific characteristics. In general, it can be stated that the siblings approach is particularly well suited if the explanatory variable of interest shows much within-family variation and that the same variable is correlated with unobservable characteristics (that are also related to the outcome) which in turn show no within-family variation.

For the purposes of this paper, the approach should not be too far from these two criteria (of which the first one is obviously fulfilled). Many of the confounding factors, both the ones that are possible to find reasonable proxies for in data (like country of origin) and those more elusive factors (like parents' ability and attitudes towards education), are permanent family characteristics.[15] Thus, the siblings approach should take care of the omitted-variable problem more effectively than a control variable approach conditioning on observables.

The siblings approach should also be a useful identifying strategy if one wants to examine the long-term impact of age at immigration, where schooling is an important intermediate mechanism. There are a few studies examining this issue (e.g., Friedberg (1993), Schaafsma and Sweetman (1999)). Schaafsma and Sweetman states: "..age-at-immigration effects do not attenuate with time in the host country, but leave permanent legacies." Poor school performance, incomplete schooling and low levels of educational attainment are all associated with worse long-run outcomes. Studying the effects of age at immigration is therefore extremely important.

Friedberg (1993) and Schaafsma and Sweetman (1999) both find a negative relation between age at immigration and earnings and the returns to education in the United States and Canada, respectively. In these kinds of studies one needs to be able to separate the, perhaps permanent, impact of age at immigration from "economic assimilation"-, cohort- and time-effects.[16] The siblings approach should be useful, since it offers a way to solve the identification problem (perfect multicollinearity) that arises in this context by implicitly netting out "economic

[15] Since the within-family variation in school affiliation is low among immigrant children in the data, time-invariant school characteristics should also be reduced when using the siblings approach.

assimilation"- and cohort-effects by studying, for example, brothers that arrive together as children and observing their outcomes at the same points in time.[17]

All results based on sibling-differences must be interpreted with the method's general limitations in mind.[18] Systematic within-family heterogeneity and interactions between family members might bias sibling-difference estimates. For example, younger children may be influenced by their older siblings in a systematic way and do worse in school than what they otherwise would have done if their older siblings had not performed less well due to their short period of residence up to graduation. This would lead to a positive bias. On the other hand, younger siblings might benefit from having older siblings. As an immigrant child, it might be beneficial per se to go through the grades as the second or third sibling than to go first.

Another concern is that parents may treat siblings differently in a way that systematically varies with their birth-order. The direction of such potential bias is of course hard to determine. Furthermore, it is only children who have siblings that identify the coefficients when using the siblings approach. Omitting the influence on the estimates from lone-children raises some concerns of how general the results are. A test of how representative the family sample is can be found in section 5.

Factors related to the outcome that change over time might affect siblings with different duration of residence differently. Changes in school policies and changes in aggregate economic conditions are such potential sources of bias.[19] On the other hand, time related factors like the fact that younger siblings benefit from their parents having resided a longer time in Sweden at

[16] Another complication when studying permanent effects using current earnings as the outcome variable is that one essentially uses current earnings as a proxy for lifetime earnings, which then is likely to induce life-cycle bias into the parameter estimates. See Haider and Solon (forthcoming) and Böhlmark and Lindquist (2005) for details.
[17] Note that the effects from age and age at immigration cannot be separated using the siblings approach if siblings migrate together. Work experience could, however, be used instead of age.
[18] Griliches (1979), Bound and Solon (1999) and Holmlund (2005) are examples of studies that discuss and analyze the properties of the within-family estimator.
[19] Sweden experienced a severe economic downturn in the early 1990's. Parents' employment status could be a factor affecting children's school performance and changes could affect siblings differently. Radical school reforms were also implemented at the time, opening up for school choice and public funding of private schools. The within-family variation in school affiliation is, however, low and so is the extent to which immigrant siblings are taught under completely different regimes.

every grade compared with older siblings are not desirable to control for. This is part of the adaptation process and should be reflected in the estimated effects from years since immigration. A family related concern in this context is, however, that many newly arrived families might first have a strong belief that they will soon return to their home country and, consequently, they invest very little in their own and their children's Sweden-specific skills. Later, as they realize that they will have to (or want to) stay permanently in their new country, they start to invest more. This would imply an overestimation of the expected negative effects from short durations of residence.

A final concern in this study is that the graduation age in a significant number of cases differs between siblings.[20] Specifically, individuals with greater difficulties in school might remain in the same level for an additional year, which introduces a selection bias in the estimation. This is, of course, a problem also in cross-sectional estimation. Sensitivity-analyses presented in section 5 examine the magnitudes of this potential problem.

3. Descriptive statistics and background analyses

Table 1 shows some important features of the data at hand. The first column shows descriptive statistics for the random sample as a whole and columns two and three give the corresponding information separately for immigrants with Western and non-Western origins. The reason for this division is that these two groups overall clearly differ in some important aspects. The Western group should, in contrast to the non-Western group, be made up of no refugees (and for this reason, children of East European origin are separated from the Western group). Human capital acquired in other Western schools is generally not so different from that acquired in Swedish schools, while this is not equally valid for non-Western schools. There may also be large cultural and linguistic differences between these two groups.

[20] Age at graduation is defined as year of graduation minus year of birth. By construction of this variable, pupils normally graduate from compulsory school at the age of 16. 19.84 percent of the children in the random sample and 19.00 percent in the sibling sample graduate at the age of 17 and a small fraction graduates at other ages clustered around these ages.

It can be noted in the first column that the overall average age at arrival is somewhat above the center of the age-span 1 to 16, and that the average unadjusted performance gap to natives in terms of GPA is 50.00 – 38.94 = 11.06 percentile units. Introducing controls for different numbers of years since immigration, the gap decreases to 4.70 percentile units. The fact that the performance gap is more than halved demonstrates the role of age at immigration as an important determinant of the gap. The differences between the Western and the non-Western groups are considerable. While the number of years since immigration appears to explain most of the performance deficit compared to natives in the latter group, it seems to have little importance in explaining the deficit of the former group.

The most striking differences between the two immigrant groups, however, are the ones in the variables "age at immigration" and "years since immigration". Western children are on average 3.33 years younger at the time of immigration than non-Western children. A comparison of the cumulative percentages of years since immigration shows this difference even clearer. For example, 67.63 percent of the non-Western children spent 9 years or less in Sweden before graduating in 9th grade, while 65.42 percent of the Western children spent 10 years or more.[21] This difference in the age at immigration pattern is in line with the discussion in the previous section about different options for refugee and labor immigrant parents to consider their children's ages when making the migration decision.

Table 2 shows the corresponding descriptive statistics for the family sample. Besides the fact that the family sample is not a random sample, there are two other important differences to keep in mind. First, the family sample includes children born in Sweden. Therefore, the average age at arrival is lower than for the random sample. Second, it includes no lone-children. Since the family sample is the identifying sample using the siblings approach, it would, nevertheless, be worrying if the general pattern shown in table 2 was very different from the one shown in table 1.

[21] The age and duration numbers presented in the table are based on information on children's latest date at arrival among those who have more than one registered. Using instead information on earliest date at arrival changes the overall numbers only marginally. However, the distribution is pushed upwards for the Western group (e.g., the cumulative percentage 20.45 for 6 years since immigration turns 16.79). Hence, the striking differences in the years since immigration distributions between the groups are further strengthened when earliest date at arrival is used. This indicates that the problem of re-migrants who turn back to Sweden again is much more of a concern when studying Western children than when studying non-Western children.

Reassuringly, a comparison reveals that the family sample does not differ significantly from the random sample if one takes into account the relatively large fraction of children that are born in Sweden and that only appears in the family sample.

We have seen that Western children on average are significantly younger when they arrive in Sweden and that they generally perform better in school than non-Western children. But is it also true that Western children perform better irrespective of their advantage of lower ages at immigration? Furthermore, how do immigrants of different origin perform in relation to the immigrant population of grade 9 pupils as a whole?

Table 3 shows cross-sectional estimates on average performance gaps between individuals in 8 subgroups by geographical origin and the individuals in the rest of the random sample, controlling for years since immigration. We see that the typical child of Western, East European or of Asian origin performs on a higher level than the average immigrant child even after controlling for years since immigration. On the other hand, an average pupil originating from South America or Africa performs on a level significantly below the others. The estimated coefficient for Middle Eastern origin is also negative, but small.

The estimates reveal a performance gap between Western and non-Western children of 3.57 percentile points. This gap is widened further to 6.20 if the Nordic children are excluded from the Western group. Regarding selection bias and the problem of unobserved cohort effects, the estimates presented in the table strengthen such concerns.

4. Results

Let us first consider cross-sectional estimates. Figure 1 shows graphed cross-sectional estimates (every fifth year cross-sections between 1988 and 2003) and pooled cross-sectional (all 16 years) estimates of the impact from different years since immigration on school performance. The trend is very similar across single year cross-sections and, thus, the approach of pooling the data seems to be plausible. Fluctuations within separate cross-sections, however, strengthen the suspicion

that cohort-effects might be driving the results. These cross-sectional estimates come from a very parsimonious specification, including no other control variable than a gender-dummy. A standard approach to try to mitigate some of the biases discussed so far would perhaps include controls for parents' socio-economic status, calendar year of immigration, country of origin and school-affiliation. The results from a more general specification, including the last three of these variables, are shown in the sensitivity-analysis in section 5.[22]

Next, we consider the sibling-difference estimates and the comparison between these estimates and the pooled cross-sectional ones. Table 4 shows estimates using both methods for all children and for girls and boys separately. Estimates from using grades in math as the outcome variable on the shortened 1998-2003 time period are also shown. In the following, y_i (where i = 1, 2, ….., 11) is used to denote different number of years since migration at time of graduation (where i > 11 is the reference alternative).

Using the sibling-difference estimator, the coefficients in column 2 reveal that children who have resided in Sweden for 7-11 years before graduation do not perform significantly worse than those who have spent a longer time in the country. This implies that children arriving during their first school years, until about the age of 10 (or until about 4th grade), seem to, at the time of graduation, have caught up with their peers who arrived in their pre-school ages. For older ages at arrival there is a relatively strong negative effect on GPA and it naturally grows stronger the less time there is left to catch up before graduating.

The same critical age at immigration is found for both girls and boys (although y_5 is small and statistically insignificant for boys). There are, however, significant differences between girls and boys in the magnitudes of the loss in GPA due to short duration of residence. Girls lose more than boys do, and the differences between the estimated coefficients are statistically significant for y_1, y_2, y_4 and y_5.[23] However, as can be noted in column 2, girls perform on a level above that of boys

[22] Even if it is possible to merge information on parents onto the working data, a satisfactory proxy for parents' pre-migration socio-economic status is probably not so easy to obtain. Immigrant adults are often missing in the education registers and income data from Sweden are an imperfect measure of individuals' pre-migration economic status (and this is what should be related to their children's age at immigration).

[23] t-ratios in absolute values for H_0: "(4) = (6) " and y1-y11: 2.71, 2.17, 1.59, 1.84, 2.98, .67, .52, .98, .70, .82, .91.

(on average 4.35 percentile points above) and it is naturally more difficult to catch up at a higher level than at a lower level.

Figure 2 displays the sibling-difference estimates for girls and boys added to their respective means obtained from pooled cross-sectional regressions controlling for years since migration. We see that the age-at-arrival-performance profiles are similar for girls and boys and that both profiles, from about $y_{7,}$ stabilize at the respective average level of performance for children arriving in Sweden in their pre-school ages. It is interesting to note that the gender gap in school performance is larger for natives than for immigrants, and that an average immigrant girl who arrives in Sweden up to about the age of 10 performs better than an average native boy.

Turning back to table 4, the differences between the sibling-difference estimates and the cross-sectional ones are considerable. First, there is no sharp break in the cross-sectional estimates at the age at immigration of 10. Instead, the negative effects on grades vanish smoothly the longer the stay in Sweden prior to graduation. Second, the negative effects are also significantly stronger for shorter duration of residence before graduation. Above the estimated critical age, the sibling-difference estimates are 27-54 percent less negative than the cross-sectional ones. Using a t-ratio test, the null hypotheses that the estimated parameters in column 1 are equal to the ones in column 2 are rejected on the 1 percent level for $y_{1,}$ $y_{2,}$ $y_{3,}$ $y_{4,}$ $y_{5,}$ $y_{7,}$ y_{8} and $y_{9.}$[24]

The profiles generated from using the two estimation approaches are also displayed in figure 3 along with 95 percent confidence intervals.[25] If one believes that the cross-sectional estimates are inflated by the kind of selection bias as was discussed above and that the sibling-difference estimates are not, then the differences observed in figure 3 are in line with what one would expect.

[24] t-ratios in absolute values for H_0: "(1) = (2) " and y1-y11: 7.14, 4.46, 4.99, 2.85, 3.59, 1.87, 2.59, 4.08, 2.97, 2.03, 1.53.

[25] In order to facilitate the comparison of the coefficients from the two different approaches in this figure and at the same time relate them to the immigrant-native performance gap, the means have been normalized using the constant from a pooled cross-sectional regression estimated with no control for gender (but with controls for number of years resided prior to graduation). The estimates from both regressions, thus, essentially converge to the (gender blind) immigrant-native gap, net the impact of age at migration, as the number of years resided in Sweden grows.

There are two previous Swedish studies that as parts of broader analyses briefly examine the role of age at immigration using cross-sectional regression approaches. Arai et al. (2000) finds that children that arrived in Sweden after the age of 7 received lower grades than other children both in math and in terms of GPA. Similä (1994) also points at the age at arrival of 7 as a border point where worse educational outcomes start to become observable. These findings are similar to the cross-sectional estimates presented here, despite the fact that these studies are performed using larger sets of control variables. This indicates that the factors that are likely to bias cross-sectional estimates can not easily be held constant using a standard control variable approach. The evidence from the siblings analysis, thus, differs significantly from both the cross-sectional estimates presented in this paper and from the evidence in previous Swedish studies using cross-sectional approaches with different specifications.

Columns 7 and 8 in table 4 show pooled cross-sectional estimates using math as the outcome variable and the shortened 1998-2003 sample. The reason that the siblings approach is not applied here is that a rather large data-range in the time dimension is required in order to obtain estimates of acceptable precision. Here, interest is only in the differences between the coefficients obtained from using math respective GPA as the outcome variables. Although the magnitudes of the estimates could be seriously biased, the bias should affect the estimates equally (and the comparison would work as well as if it was done using the siblings approach).

The estimates in the table reveal that short duration of residence is significantly less harmful to the performance in mathematics than it is to the average performance in a range of subjects. The differences are statistically significant at the 1 percent level for y_1-y_3 and at the 10 percent level for y_4. This demonstrates the importance of Sweden-specific skills as an important factor explaining the performance gaps between born abroad children with different duration of residence in the country prior to graduation (and between children born abroad and natives).[26]

[26] Note that there is no significant gender difference for immigrant children in mathematics. This is also observed for the population of pupils as a whole in the Swedish National Agency for Education (2004).

Turning to the separate regressions by geographical origin, table 5 reveals that there are striking differences by country of origin. Pupils of Western origin do not have any statistically significant disadvantage even from having a very short time in the country prior to graduation. This indicates that there is not much to catch up, i.e., that the human capital they have acquired in the source country does not differ much from that acquired in Sweden. Recall from table 1 that the Western immigrant pupils perform on a level just below that of natives and that the number of years resided does not explain much of the gap there is. Another way to view this is that the y-values are not assigned by chance, least of all among Western children whose parents most often have the option to choose the timing of the migration. Parents decide on the timing with respect to their children's individual ability to handle the new environment. For some years since immigration we actually see that the estimates are significantly positive.

Children of Asian origin also perform on a high average level, but in contrast to Western children they lose much from having few years in Sweden before graduation. It is not likely that older siblings should be systematically less able or less ambitious. Thus, the steep average age-at-immigration-performance profile observed for the Asian group is instead likely to reflect large differences in Sweden specific skills between children with relatively high respective relatively low age at migration. Children of East European origin also perform on a high average level. Not surprisingly, the estimated age-at-immigration-performance profile is very much like the one for Western children.

Among the groups that perform on a level below the average in the immigrant population of 9th grade graduates as a whole, there are also striking differences. The slope of the age-at-immigration-performance profile for South American children is more like that for Western and East European children, while the slopes of the profiles for African and Middle Eastern pupils are steeper and more similar to the one estimated for the Asian group. The age-at-immigration-performance profiles are displayed graphically in figure 4.

Figure 4 makes it clear how the groups differ in both average performance (at those ages at immigration that seem to have no negative impact) and in the slopes of the age-at-immigration-performance profiles. The six profiles are also similar in that they all seem to having converged

to their respective mean for children with $y > 11$ at around y_7. The critical age at arrival of about 10, thus, seems to be rather stable for the division into sub-groups, although the profiles for Western, East European and South American children converges to their respective means below y_7.

Differences in slopes and differences in levels give rise to different questions. Concerning the steep slope of the age-at-immigration-performance profile for Asian children: why do the children arriving in Sweden when they are older than 10 perform so much worse than their peers arriving at younger ages? Answers to this question may induce school polices specially directed to this group in order to facilitate the adapting process (by perhaps improved teaching in Swedish, based on knowledge about linguistic differences). Concerning the flat profile for South American children: why do the pupils arriving in Sweden before the first grade perform about as bad as their peers arriving only some few years before graduation? Perhaps this reflects a low general level of motivation? Hence, the slopes of these age-at-immigration-performance profiles might reflect different phenomena requiring different interventions from policy-makers that are interested in closing the overall immigrant-native performance gap. It is not the scope of this paper to discuss level differences between immigrant groups and between immigrants and natives not associated with the role of age at migration. Reducing the differences in school performance that are due to other factors is likely to involve other policies than only school policies.

5. Sensitivity analysis

Are there any other possible explanations than unobserved cohort effects and selection for the difference between the years since immigration coefficients in the sibling-difference and the cross-sectional analyses? This section examines some particular concerns.

The first concern to examine is if the family sample is so different from the representative random sample that this explains some of the observed differences. In particular: is it the case that essential information falls out when the lone-children are omitted, and is the sample much less representative when it in addition includes siblings to the randomly selected children? An

attempt to answer these two questions is made by running cross-sectional regressions, first excluding lone-children from the random sample and then using the full family sample. Second, the main point with using the siblings approach is that it should net out cohort-effects and effects due to selection more efficiently than a cross-sectional approach. All the parameter estimates presented thus far have been obtained from regression equations in which no attempt has been made to mitigate likely biases by including plausible control variables. In this section, one such attempt is made by extending the baseline pooled cross-sectional regression with controls for country of origin, calendar year of immigration and school affiliation.

The results from these sensitivity analyses are displayed in figure 5. It is evident that excluding lone-children from the baseline random sample does not lead to cross-sectional estimates that differ much from the baseline. The fact that lone-children are excluded from the family sample (as they identify no coefficients in sibling-difference estimation), thus, does not seem to be of any concern. Likewise, cross-sectional parameter estimates using the family sample (random sample minus lone-children plus siblings) are not significantly different from the baseline either.[27] The control variable approach generates estimates that overall lies above the baseline, closer to the sibling-difference estimates, but the difference in comparison to the baseline is small in magnitude and statistically insignificant.

Another concern is that by allowing individuals in the two identifying samples (random and family) to differ from the normal age of graduation, the estimates potentially also reflect self-selection bias. For example, children with greater difficulties in school might remain at the same grade for an additional year. In opposite to selection associated with the migration decisions made by parents, this selection problem pertains to both estimation approaches. Cross-sectional and sibling-difference estimates from excluding individuals that differ from the normal age of graduation are presented in figure 5 and 6, respectively.

Figure 5 reveals that these cross-sectional coefficients are below the baseline for ages at immigration close to 16 and above the baseline for lower ages at immigration. Again, the

[27] To get a feel for statistical significance, compare with figure 2 in which confidence intervals around the baseline cross-sectional estimates are displayed.

differences from the baseline are not statistically significant, but still this evidence suggests that the baseline regression underestimates the negative effects due to short duration of residence. This is the expected direction of bias due to this selection problem. The corresponding sibling-difference parameters, displayed in figure 6, relate to the baseline in a similar way as the cross-sectional estimates do.

Another possible explanation for the observed differences between the two approaches is measurement error bias which was also discussed in section 2. There, it was argued that since the measurement errors in date of immigration most commonly do not vary within the same family, the "noise" is a fixed effect when estimating sibling-differences. Following this argument, the cross-sectional estimates should be more affected by measurement error bias than the sibling-difference estimates. About 12 percent of the families in the family sample, however, include siblings differing in calendar year at arrival. Is it the case that within-family heterogeneous measurement errors in date at arrival among these families drive the sibling-difference estimates? This is examined in figure 6 by excluding these families from the baseline sample.

A final and similar concern is that there might be a measurement problem also regarding some of the families that are registered as having arrived the same calendar year. Individuals in data are sometimes registered as having arrived in Sweden more than once. This reflects that families have re-migrated and then returned to Sweden. Once again, since the individuals studied are children, this should not be a major concern because they migrate along with their parents and the errors that follow from choosing either their earliest or latest (or mean) year at immigration implies family-specific errors. However, some children are born in Sweden and might not even be born when their sibling or siblings experience their first stay in the country. Also, families might be split for a time and then re-unified in Sweden. Are the sibling-difference estimates sensitive to the choice of using individual information on earliest or latest year of arrival? This is examined by changing from the baseline-choice of latest calendar year of immigration to the use of the earliest. As the phenomenon that re-migrants return to Sweden is most common among the Western immigrant group, this part of the sensitivity analysis is performed also separately for them.

Figure 6 shows that none of these sensitivity analyses generate estimates that differ to any significant extent from the baseline ones.[28] Hence, families whose children have different dates at arrival registered do not seem to induce measurement error bias into the estimates. Likewise, for the full family sample, the choice of using information on individuals earliest or latest registered date of arrival does not seem to matter either. For the estimation using Western families only, on the other hand, this choice does seem to matter. The general patterns displayed for this group are, however, almost the same for both choices and do not change the conclusion that age at immigration is not an important factor explaining Western pupils' school performance.

To sum up, the sensitivity analyses performed in this section give no evidence that the differences observed between the baseline estimates from the two approaches are driven by flaws in the siblings analysis or flaws in the comparison. If, in addition, it is the case that the cross-sectional estimates in part reflect measurement error bias and one assumes that this attenuates the estimated coefficients, then the true differences, ceteris paribus, between the sibling-difference estimates and the cross-sectional ones are even larger.

6. Conclusion

The purpose of this study has been to examine critical ages at immigration for school performance and to estimate average-age-at-immigration-performance-profiles for different groups of immigrant children graduating from 9th grade. The analysis was performed exploiting within-family variation in a large set of high quality Swedish register data on immigrant siblings and native children. This approach has several advantages over a standard cross-sectional identification strategy. In particular, cohort effects and omitted-variables related to the migration decision made by parents should be netted out when estimating sibling-differences. The sibling estimates in this study are also less prone to measurement error bias than the benchmark cross-sectional ones.

[28] To get a feel for statistical significance, compare with figure 2 in which confidence intervals are displayed.

The main findings can be summarized as follows. First, the estimated critical age at immigration is about 10. Thus, children arriving in the country up to about the 4th grade seem to be catching up well with their peers either born in Sweden or immigrating before the start of preschool. This result is stable for both boys and girls and for children of different origin. Second, any similar marked critical age at immigration is not found using cross-sectional estimation, and above the age of 10 the sibling-difference estimates are 27-54 percent less negative than the cross-sectional ones. This is an expected pattern if the cross-sectional estimates are inflated by selection bias due to parents' migration decisions. Third, as with natives, immigrant girls outperform immigrant boys (immigrant girls arriving in Sweden up to about the 4th grade also perform on a level above that of native boys). The negative effects due to short duration of residence are significantly larger for girls than for boys, although the slopes of the age-at-immigration-performance profiles are similar. Fourth, there are striking differences between immigrants from different source areas. Children of Western, East European and Asian origins outperform other immigrant children. The steepest average age-at-immigration-performance-profiles, on the other hand, can be observed for children of African, Middle Eastern and Asian origins. Fifth, few years in a Swedish school prior to graduation is significantly less negative for the performance in mathematics than it is in terms of GPA which demonstrates the importance of Sweden-specific skills.

Special directed school resources could provide effective means to close the overall native-immigrant school performance gap. The findings presented in this paper suggest that such resources would have the potentially largest positive effect if they were targeted to children arriving in Sweden around the age of 10 or above, and especially so if they were targeted to children originating from African, Middle Eastern or Asian countries. This piece of policy advice is, however, strictly focused on the targeting of means to speed up the adapting process among children that evidently are disadvantaged due to short duration of residence. The relatively flat age-at-immigration-performance profile and the low level of average performance observed for South American children might for instance call for other interventions that could also be efficiently targeted.

Finally, this study has only examined short-term effects. A natural question to be answered by future research is if these effects are temporary or permanent, i.e., if the children arriving in the

country after the age of 10 are catching up with their younger peers in terms of long-run outcomes or if the gap persists into adult ages. The siblings approach should be useful also in such a study.

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Table 1 ***Descriptive statistics, 1988-2003 pooled observations for the random sample***

	All	Western	Non-Western
Variable	*Mean*	*Mean*	*Mean*
Age at arrival	8.29	5.45	8.78
Position in the natives grade distribution[a]:			
unadjusted	38.94	44.71	37.93
adjusting for years since immigration	45.30	45.59[b]	45.16[c]
Years since immigration at time of graduation:	*Frequencies*	*Cum. percentages*	*Cum. percentages*
1 or less	979	2.92	5.93
2	1,104	6.02	12.64
3	1,195	9.23	19.94
4	1,189	12.49	27.18
5	1,298	16.00	35.10
6	1,359	20.45	43.26
7	1,215	23.96	50.63
8	1,314	28.45	58.48
9	1,555	34.58	67.63
10	1,355	40.86	75.44
11	1,112	47.70	81.55
12 or more	4,204	100	100
Number of individuals	17,879	2,675	15,204

Notes: [a]The ranking distribution (Percentile ranks of grade point averages from 9th grade) is evaluated in a 20 percent random sample of the Swedish native population. Because it is percentile ranks, the mean is 50.00. [b]Nordic immigrants: 43.39, other Western immigrants: 49.28. [c]Asian immigrants: 52.99, East European immigrants: 48.32, South American immigrants: 38.30, African immigrants: 44.00. Middle Eastern immigrants: 45.34.

Table 2 ***Descriptive statistics, 1988-2003 pooled observations for the family sample***

	All	Western	Non-Western
Variable	*Mean*	*Mean*	*Mean*
Age at arrival	7.26	3.94	7.75
Position in the natives grade distribution[a]:			
unadjusted	39.03	46.00	38.01
adjusted for years since immigration	43.60	45.50[b]	42.89[c]
Years since immigration at time of graduation:	*Frequencies*	*Cum. percentages*	*Cum. percentages*
1 or less	660	1.24	3.25
2	936	3.01	7.86
3	1,157	4.61	13.64
4	1,297	6.70	20.08
5	1,487	9.29	27.44
6	1,516	12.90	34.80
7	1,605	16.20	42.66
8	1,614	19.43	50.58
9	1,872	24.14	59.63
10	1,711	29.81	67.70
11	1,498	35.31	74.68
12 or more	6,692	100	100
Number of individuals	22,045	2,821	19,224

Notes: [a]The ranking distribution (Percentile ranks of grade point averages from 9th grade) is evaluated in a 20 percent random sample of the Swedish native population. Because it is percentile ranks, the mean is 50.00. [b]Nordic immigrants: 43.23, other Western immigrants: 48.90. [c]Asian immigrants: 54.14, East European immigrants: 45.90, South American immigrants: 37.46, African immigrants: 44.19, Middle Eastern immigrants: 42.06.

Table 3 ***Differences in school performance by geographical origin, controlling for years since immigration***

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummies for the respective origin:								
Western	3.57*							
Western, non-Nordic		6.20*						
Nordic			0.96					
East European[b]				5.60*				
Asian					1.77*			
South American						-8.05*		
African							-7.60*	
Middle Eastern								-1.70*
Female	5.03*	5.06*	5.04*	5.02*	5.03*	5.13*	5.04*	5.00*
Constant	41.68*	42.08*	42.66*	42.14*	42.75*	43.93*	43.26*	43.36*
Controls for years since immigration	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of individuals	17,879	17,879	17,879	17,879	17,879	17,879	17,879	17,879
Adjusted R-squared	0.116	0.117	0.114	0.123	0.114	0.123	0.120	0.115

Notes: Dependent variable: Percentile ranks of grade point averages from 9th grade. t-statistics are based on robust standard errors. *significant at the 1 percent level. [x]significant at the 5 percent level. [+]significant at the 10 percent level. [b]Including immigrants from former European communist countries and in addition from Belarus, Ukraine, Russia and the Baltic states.

Table 4 ***Estimates of the effects of years since immigration on school performance***

	All	All	Girls	Girls	Boys	Boys	All	All
Dep.Variable / *Variable*	GPA (1)	GPA (2)	GPA (3)	GPA (4)	GPA (5)	GPA (6)	GPA (7)	Math (8)
Years since immigration (y) :								
1 or less	-31.75*	-22.68*	-32.99*	-26.90*	-30.58*	-19.85*	-31.98*	-24.99*
2	-20.59*	-14.98*	-21.61*	-19.27*	-19.75*	-14.27*	-22.10*	-15.01*
3	-13.86*	-7.93*	-15.99*	-10.73*	-11.86*	-7.29*	-14.84*	-9.01*
4	-9.85*	-6.68*	-9.22*	-9.09*	-10.39*	-5.47*	-11.56*	-8.31*
5	-7.16*	-3.28*	-7.73*	-6.24*	-6.63*	-0.69	-7.31*	-6.93*
6	-4.84*	-2.86*	-3.95*	-3.79*	-5.58*	-2.55[×]	-5.41*	-4.80*
7	-2.79*	-0.01	-3.10*	0.04	-2.51*	0.96	-2.90*	-4.51*
8	-3.25*	0.98	-2.74[×]	-0.00	-3.72*	1.66	-4.02*	-5.17*
9	-2.64*	0.25	-2.05[×]	-0.05	-3.24*	1.08	-2.64*	-2.80*
10	-1.19	0.84	-1.96[+]	0.13	-0.48	1.40	-1.33	-2.14[×]
11	-1.76[×]	-0.16	-2.12[+]	0.69	-1.42	-0.77	-2.34[×]	-3.62*
12 or more	Ref. alt	Ref. alt	Ref. alt	Ref. alt	Ref. alt	Ref. alt	Ref.alt	Ref.alt
Model	C.S.	sib diff	C.S.	sib diff	C.S.	sib diff	C.S.	C.S.
Female	5.05*	4.35*					5.49*	0.07
Constant	42.86*	39.33*	48.13*	43.43*	42.66*	38.19*	42.82*	44.09*
Number of individuals	17,879[a]	22,045[b]	8,567[c]	7,083[d]	9,312[e]	7,947[f]	9,039[g]	9,039[g]
Number of families		8,263		2,992		3,461		
Adjusted R-squared	0.114	0.511	0.110	0.529	0.100	0.509	0.108	0.045

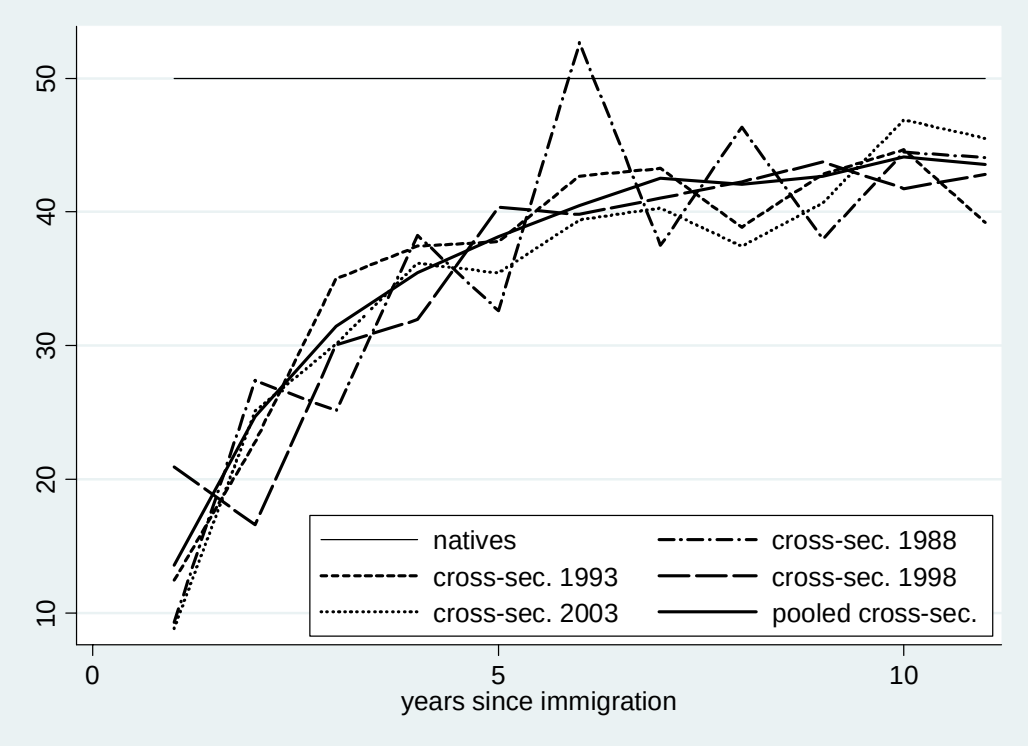
Notes: Dependent variables in columns 1-7 are the percentile ranks of grade point averages from 9th grade. Dependent variable in column 8 is the percentile ranks in mathematics from 9th grade. Family controls are dummy-variables implicitly estimated in the regression models estimating fixed family effects. t-statistics are based on robust standard errors. *significant at the 1 percent level. [×]significant at the 5 percent level. [+]significant at the 10 percent level. **Samples:** [a]full random sample. [b]random sample + siblings to randomly sampled individuals – "lone children in the random sample". [c]girls from the random sample. [d]girls from the random sample + sisters to randomly sampled girls – "lone children in the random sample" – girls who only have brothers in the sample. [e]boys from the random sample. [f]boys from the random sample + brothers to randomly sampled boys – "lone children in the random sample" – boys who only have sisters in the sample. [g]full random sample for the years 1998-2003.

Table 5 ***Estimates of the effects of years since immigration on school performance by geographical origin***

Variable	Western (1)	East[a] European (2)	Asian (3)	South American (4)	African (5)	Middle Eastern (6)
Years since immigration at time of graduation:						
1 or less	-6.54	-14.62*	-40.91*	-16.86*	-28.28*	-26.40*
2	1.37	-8.86*	-29.97*	-14.15*	-24.86*	-16.42*
3	8.57[×]	-1.87	-16.29*	-4.33	-17.86*	-10.13*
4	3.82	-1.10	-15.85*	-4.10	-14.00*	-8.14*
5	6.51[×]	2.15	-9.21*	-2.22	-7.45*	-5.19*
6	3.30	1.49	-11.69*	2.04	-10.29*	-3.25*
7	3.05	4.76*	-0.36	0.50	-4.84[+]	-1.52
8	7.07*	3.70[×]	-1.60	1.95	-4.30	0.53
9	4.40[+]	1.85	0.13	-0.20	-1.41	0.02
10	2.52	2.04	-1.09	0.68	-2.87	0.74
11	0.77	-0.02	-2.50	1.90	-1.32	-0.62
12 or more	Ref. alt	Ref. alt	Ref. alt	Ref. alt	Ref. alt	Ref. alt
Model	sib diff	sib diff	sib diff	sib diff	sib diff	sib diff
Female	5.70*	5.15*	4.42*	2.68*	2.26*	4.18*
Constant	42.13*	36.26*	48.86*	33.28*	39.57*	39.13*
Number of individuals[b]	2,821	4,613	1,386	2,121	1,439	9,663
Number of families	1,137	1,962	488	832	490	3,353
Adjusted R-squared	0.510	0.566	0.520	0.451	0.478	0.486

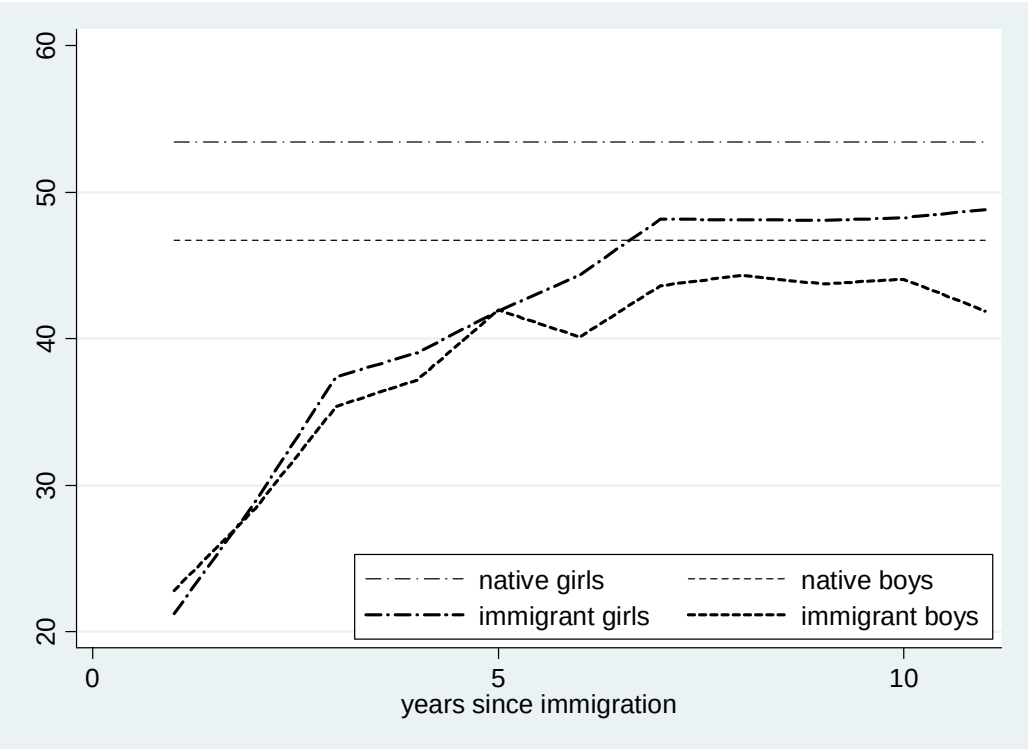
Notes: Dependent variable: Percentile ranks of grade point averages from 9th grade. Family controls are dummy-variables implicitly estimated in these regression models, estimating fixed family effects. t-statistics are based on robust standard errors. *significant at the 1 percent level. [×]significant at the 5 percent level. [+]significant at the 10 percent level. [a]Including immigrants from former European communist countries and in addition from Belarus, Ukraine, Russia and the Baltic states. **Samples for the respective origin:** [b]random sample + siblings to randomly sampled individuals – "lone children in the random sample".

Figure 1 ***1988, 1993, 1998, 2003 and pooled 1988-2003 cross-sectional estimates of the impact of years since immigration on school performance***



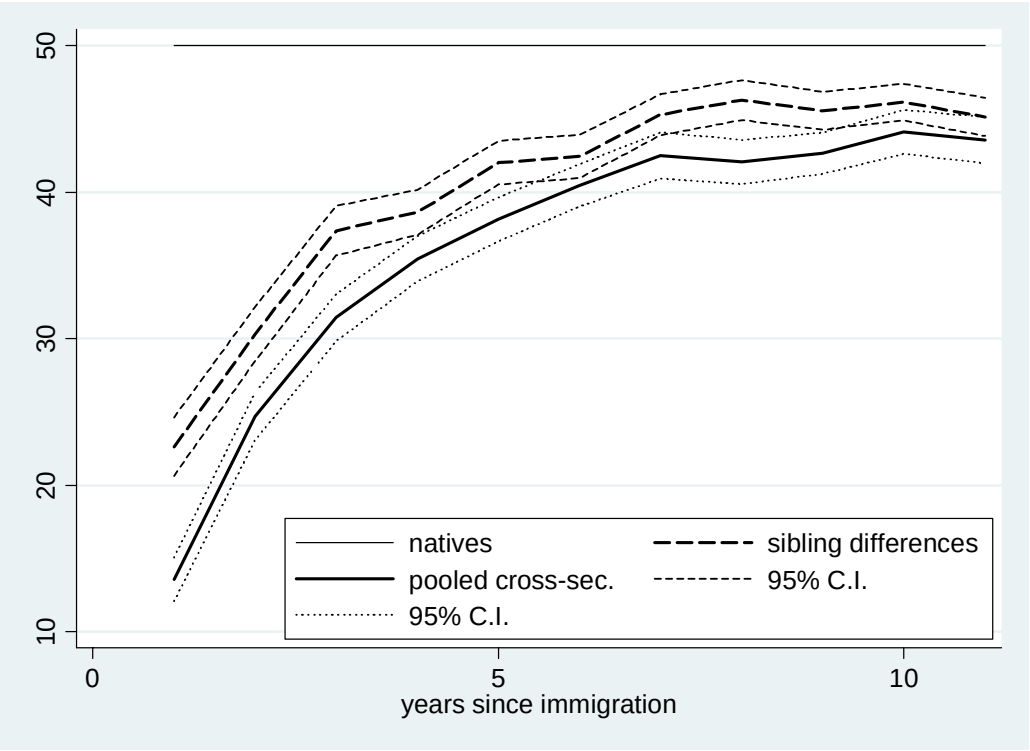
Note: The cross-sectional estimates in the figure converge to the average level of relative performance among immigrant children with a residence in Sweden exceeding 11 years.

Figure 2 ***Sisters- and brothers-difference estimates of the impact of years since immigration on school performance***



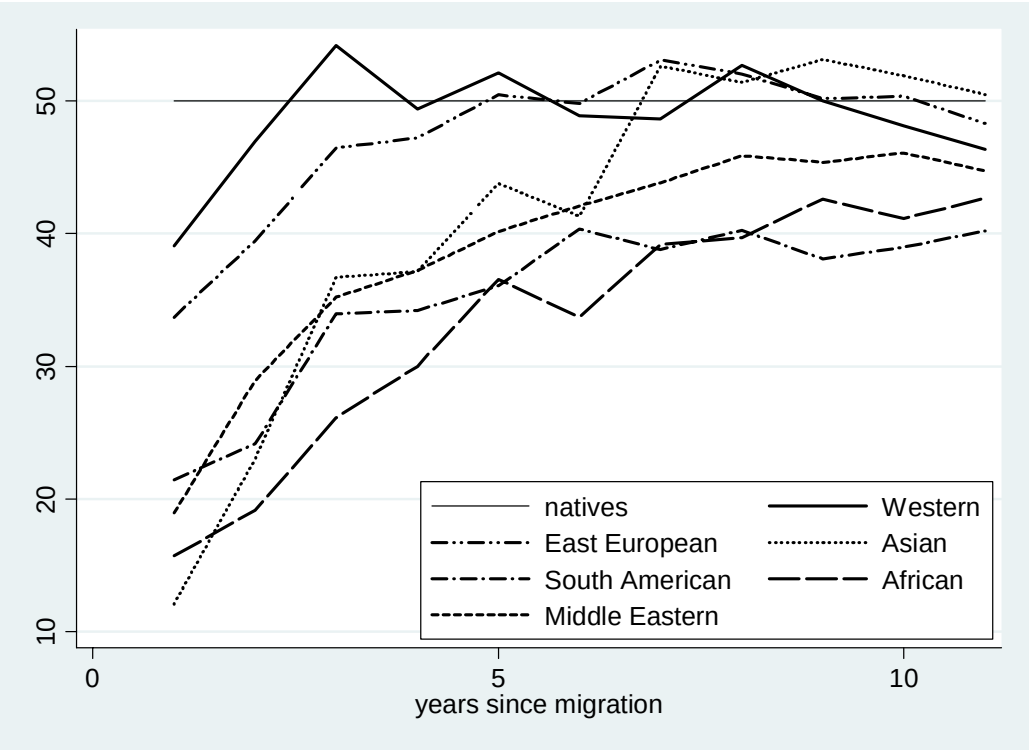
Note: The sibling-difference estimates in the figure converge to the respective average level of relative performance among immigrant girls and boys with a residence in Sweden exceeding 11 years.

Figure 3 ***Sibling-difference and pooled 1988-2003 cross-sectional estimates of the impact of years since immigration on school performance***



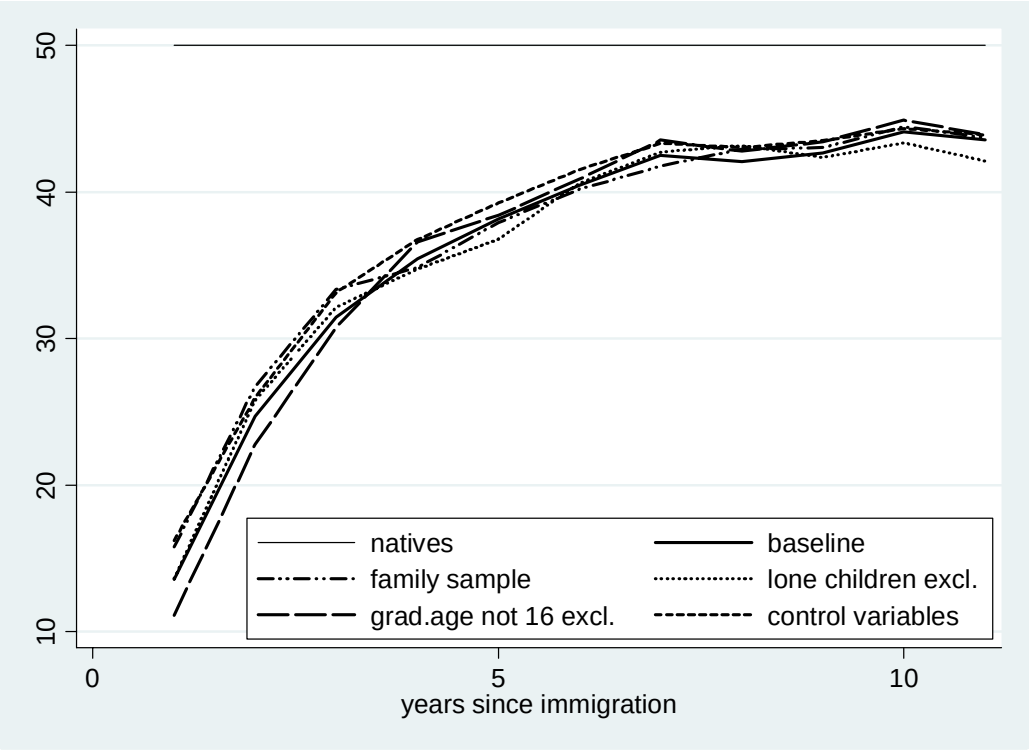
Note: The sibling-difference and the pooled cross-sectional estimates do both converge to the average level of relative performance among immigrant pupils with a residence in Sweden exceeding 11 years.

Figure 4 ***Sibling-difference estimates of the impact of years since immigration on school performance for children with different origin***



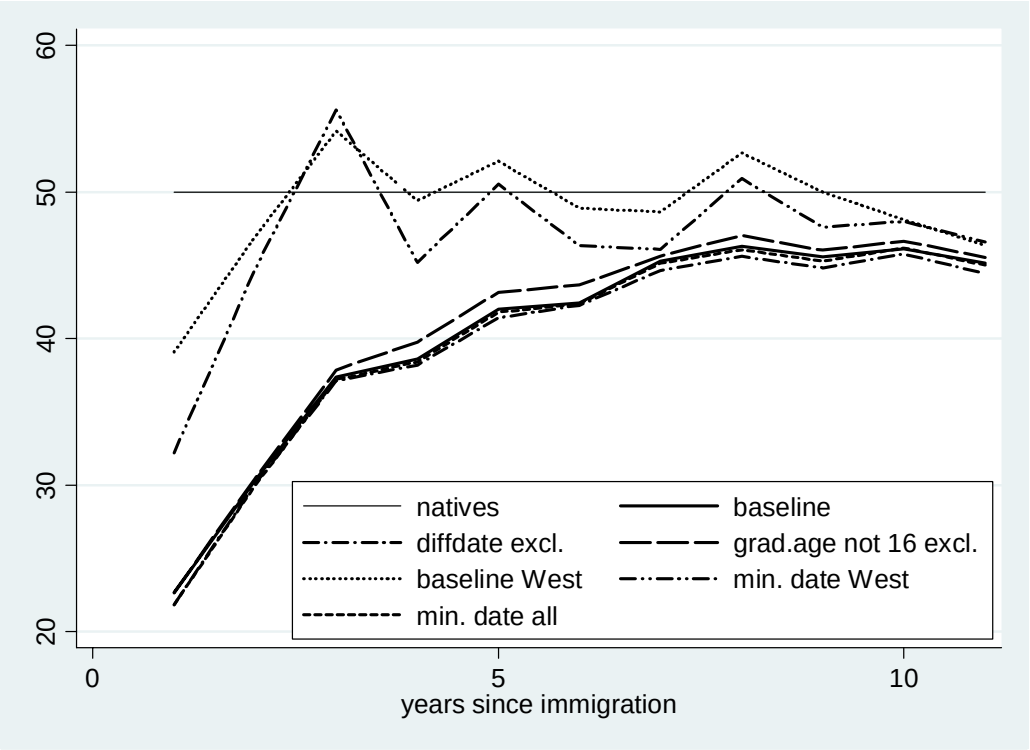
Note: The estimates in the figure do all converge to the respective origin-specific average level of relative performance among children with a residence in Sweden exceeding 11 years.

Figure 5 ***Sensitivity analyses of the pooled cross-sectional estimates***



Notes: The baseline is the pooled cross-sectional estimates obtained using the random sample. "family sample" is corresponding estimates obtained using the family sample. "lone children excluded" is corresponding estimates obtained using the random sample but with lone children excluded from it. "grad.age not 16 excl." is corresponding estimates obtained using the random sample but excluding individuals graduating from compulsory school at ages different from the normal one of 16. "control variables" is estimates obtained from a regression using the random sample but in addition to the baseline controlling for school affiliation and country of origin.

Figure 6 ***Sensitivity analyses of the sibling-difference estimates***



Notes: The baseline is the sibling-difference estimates obtained using the family sample. "diffdate excluded" is corresponding estimates obtained excluding families that demonstrate sibling-differences in calendar year of immigration. "grad.age not 16 excl." is corresponding estimates obtained using the family sample but excluding individuals graduating from compulsory school at ages different from the normal one of 16. "min. date all" is corresponding estimates obtained using the baseline family sample but using information of children's earliest date at arrival instead of the latest. "baseline West" and "min. date West" corresponds to "baseline" and "min. date all" but are estimates based on the sample of Western families alone.